functions of the cortex alone; also, that the association of ideas is secured by the intimate union of various areas of the cortex, through the medium of nerve fibres passing just beneath the surface.

In view of the remarkable work which is being done at present in the department of psycho-physics in measuring the time of such processes of association, the study of the physical basis of the physiological process gains in interest.

The physiologists have succeeded in demonstrating the complex organic basis of memory by these experiments upon the cortex. It is now evident that we must speak rather of memories than of memory—each sensory or motor act leaving behind it a molecular change in the cortex which is to be regarded as the physical substratum necessary to recollection or reproduction. And as the memory of any single object is made up of a number of revived impressions, each derived through a separate sense, and each received in a different area, the mental image of the object involves the activity of various parts of the cortex, the revival of numerous, distinct memory-pictures, joined in a complex unit. It follows that a single set of memories may be lost by disease in one part of the brain, while other memories remain, a conclusion which is amply illustrated in the phenomena of aphasia. (Ferrier, pp. 440–460).

That there is any necessity for postulating "ideational centres" distinct from the correlated sensory and motor centres, is combatted by Ferrier. "We have in the sensory and motor centres of the cortex the substrata of the respective forms of sensory perception and ideation, and of the individual acts of volition, simple and compound, as well as of the feelings associated with their activity. It seems more reasonable to suppose that there may be higher and lower degrees of complexity or evolution in the same centres than to assume the separate existence of more highly evolved centres for which no evidence is obtained by the results of experimental research." (Page 460.) "Intelligence and will have no local habitation distinct from the sensory and motor substrata of the cortex generally. There are centres for special forms of sensation and ideation, and centres for special motor activities and requisitions, in response to and as association with the activity of sensory centres; and these, in their respective cohesions, actions and interactions, form the substrata of mental operations in all their aspects and all their range." (Page 467.)

The discussion of the psychological side of brain action is more intelligent and philosophical in the English than in the Italian work. But both of these books may be recommended for careful perusal to anyone who desires to become familiar with the facts upon which the theory of the localization of brain functions is based.

M. Allen Starr.

*Francis Galton on the Persistency of Type.*

In his opening address as President of the Anthropological section of the British Association, at its Aberdeen meeting, Francis Galton gave an account of his researchesregarding the inheritance of size in seed and of stature in man, as well as certain generalizations which he deduces from his observations. His observations

have been warmly and justly welcomed by all students of inheritance, as valuable contributions to our positive knowledge of a subject where the attainment of positive knowledge is peculiarly difficult; but it seems to me that, although the general conclusions are worded with the greatest care, they are in some respects misleading, and opposed to our general knowledge of the subject.

A few extracts will serve to exhibit the results of his observations and the character of his general deductions. He says (Nature, Sept. 4, 1885): "It is some years since I made an extensive series of experiments in the produce of seeds of different sizes, but of the same species. * * * It appears from these experiments that the offspring did *not* tend to resemble their parent seeds in size, but to be always more mediocre than they; to be smaller than they if the parents were large; to be larger than the parents if the parents were very small," and that the analysis of the family records of the heights of 205 human parents and 930 children fully confirms and goes far beyond the conclusions he obtained from seeds, as it gives with great precision and unexpected coherence the numerical value of the regression towards mediocrity. He points out that this regression is a necessary result of the fact that "the child inherits partly from his parents, partly from his ancestors. Speaking generally, the further his genealogy goes back, the more numerous and varied will his ancestors become, until they cease to differ from any equally numerous sample taken at haphazard from the race at large. Their mean stature will then be the same as that of the race; in other words, it will be mediocre." He illustrates this by comparing the result of the combination in the child of the mean stature of the race with the peculiarities of its parents, to the result of pouring a uniform proportion of pure water into a vessel of wine. It dilutes the wine to a certain fraction of its original alcoholic strength, whatever that strength may have been.

He then goes on to conclude that the law of regression to the type of the race "tells heavily against the full hereditary transmission of any rare and valuable gift, as only a few of many children would resemble their parents. The more exceptional the gift, the more exceptional will be the good fortune of a parent who has a son who equals, and still more if he has a son who surpasses him." The law is even-handed; it levies the same heavy succession tax on the transmission of badness as well as goodness. If it discourages the extravagant expectations of gifted parents that their children will inherit all their powers, it no less discountenances extravagant fears that they will inherit all their weaknesses and diseases." * * * "Let it not be supposed for a moment that" the "figures invalidate the general doctrine that the children of a gifted pair are much more likely to be gifted than the children of a mediocre pair; what it asserts is that the ablest children of one gifted pair is not likely to be as gifted as the ablest of all the children of many mediocre pairs."

My first criticism of Galton's data as a basis for generalization is that they are misleading in so far as they fail to discriminate between the persistency of hereditary and that of non-hereditary parental peculiarities.

The departure of a human being from the normal stature of the race may be an example of any one of three classes of phenomena:

1. It may be physiological, or due to influences which during the life of the individual tend to dwarf or to develop it. The short

stature of sailors is usually attributed to this cause, and it is possible that the size of the seeds and the stature of some of the human parents was physiological. In this case there is no reason to expect any tendency towards perpetuation.

2. A variation may be congenital but not hereditary, as when a single giant or dwarf is born in a family where the ancestors and descendants have the normal stature.

3. The peculiarity may be congenital and hereditary, as it is when a certain stature is characteristic of the brothers, sisters, and colateral relatives of a parent; when it is a family characteristic, or when it is characteristic of a variety of the human race, like the Bushmen.

There is ample evidence that the persistency, in the descendants, of a parental peculiarity varies greatly according as it belongs to one or the other of these classes, and we know that, quite independently of any selection, a hereditary peculiarity—that is, one which is shared by all the members of a family—often shows an astonishing tendency to persist in later generations, quite independently of the time during which it has already persisted.

A most remarkable illustration may be found on page 30 of Professor Bell's memoir on "The Formation of a Deaf Variety of the Human Race." (Mem. Nat. Acad. of Sc., Nov., 1883).

In the H. family, of Kentucky, two brothers and a sister inherited from their parents a common predisposition towards deafness, as is shown by the fact that they all became the ancestors of congenital deaf mutes, although only one of them was deaf. We have no information regarding the first generation, the parents, but in the second generation one of the three children was deaf. In the third generation all the descendants, eleven in number, were deaf. In the fourth generation the record is incomplete, but all the children which are known, six in number, were deaf. In the fifth generation selection was introduced, as three of the children married deaf mutes. The records are very incomplete, but of the six descendants known one was deaf.

The genealogy of this family is given in the following table, which serves to show that, in case of a hereditary peculiarity, the tendency of the children to resemble their parents may be vastly greater than their tendency to revert to the normal type of the race.

<table>
<tr><td>First generation.</td><td colspan="5">No information concerning their hearing.</td></tr>
<tr><td>Second generation.</td><td>Son deaf.</td><td>Daughter hearing.</td><td colspan="3">Daughter hearing.</td></tr>
<tr><td>Third generation.</td><td>Seven deaf children.</td><td>Two deaf children.</td><td colspan="3">Two deaf sons.</td></tr>
<tr><td rowspan="2">Fourth generation.</td><td rowspan="2">No information concerning the descendants.</td><td rowspan="2">One child had two deaf children; no information concerning the other.</td><td colspan="3">One son did not marry; the other had two deaf daughters, $D,^1$ $D,^2$ and one deaf son, S.</td></tr>
<tr><td>$D^1$ married a deaf man.</td><td>$D^2$ married a deaf man.</td><td>S married a deaf woman.</td></tr>
<tr><td>Fifth generation.</td><td>No information.</td><td>No information.</td><td>One deaf son.</td><td>No children.</td><td>Five hearing children.</td></tr>
</table>

I find among some notes which Professor Bell has kindly placed in my hands another interesting case. O. H. was the only deaf child in a family of eleven children. He had four children, two of them deaf, and three grandchildren, two of them deaf, so that the relative predisposition of his parents, himself and his children to transmit deafness may be represented by three fractions, $\frac{1}{11}$, $\frac{1}{2}$, $\frac{2}{3}$.

It is only in a figurative sense that we can say that a child is the offspring of remote ancestors as distinguished from its parents; for even if we believe in the continuity of germinal protoplasm, it still remains true that all the matter in the fertilized egg comes from the parents, and the history of the Kentucky family shows that a hereditary variation, even when it is not very ancient, may be much more potent than all the influence which comes from ancestry.

These facts, and many more which might be quoted from our stock of information regarding domesticated animals and plants, show that if Galton had studied the persistency of *hereditary* peculiarities of stature, independently of selection, his results might have been quite different, and the experience of all breeders shows that if he had tabulated by themselves the cases where the parents had the *same* hereditary peculiarity of stature, where selection had been exercised, his general conclusion would be quite inapplicable to the result.

A few months after Galton's paper was printed another paper appeared by a well-known authority (Die Bedeutung der sexuellen Fortpflanzung für die Selections-Theorie, Weismann, Jan., 1886), and on page 40 I find the statement that "when the same part is greatly developed in both parents, the experience of breeders shows that it is still more developed in the children."

It is undoubtedly true that the average child is less aberrant than the parents, and that each child inherits a tendency to revert, or, as Galton shows, to lie midway between its parents and the type of the race; but it is also true that when both parents have the same peculiarity there is a very considerable probability that some children will equal or surpass them, so that the peculiarity may be rapidly culminated by selection.

Galton overlooks the fact that the "type" itself is not a fixed quantity, since it admits of rapid modification by the continual selection of such slight variations as constantly present themselves under the ordinary and normal conditions of life.

It seems to me that the following observations disprove his statement that "the appearance of a new type is due to causes beyond our reach," as they show that the type, that is, "*the ideal form towards which the children of those who deviate from it tend to regress*" (*Galton*) may itself be rapidly modified by selection.

The observations are given by Fritz Müller in a recent number of Kosmos (Ein Zuchtungsgesuch an Mais, Kosmos, 1886, 2, 1, p. 22).

Yellow corn is very variable in many respects. The number of rows of kernels on the cob is usually from 8 to 16, cobs with 10 or 12 rows being the most common, while one with 18 or 20 rows is very seldom found. A search through several hundred cobs gave him one with 18 rows, but none with more.

In 1867 he sowed at different times, and in such a way as to prevent crossing, (1) the seed from the cob with 18 rows, (2) the seed from the finest 16 rowed ears, and (3) the seed from the finest 14 rowed ears. In 1868 he sowed (1) seed from a 16 rowed ear

which had grown from seed from a 16 rowed ear, (2) seed from an 18 rowed ear from 16 rowed seed, and (3) seed from an 18 rowed ear from 18 rowed seed.

In 1869 he sowed (1) seed from an 18 rowed ear with 18 rowed parents and grandparents, (2) seed from a 20 rowed ear with 18 rowed parents and grandparents, and (3) seed from a 22 rowed ear from seed from an 18 rowed ear, produced from seed from a 16 rowed ear. The results are given in the accompanying table:

| | 1867. | | | 1868. | | | 1869. | | |
|---|---|---|---|---|---|---|---|---|---|
| Number of rows on cob from which *seed* was taken.... | 14 | 16 | 18 | 16 16 | 16 18 | 18 18 | 18 18 18 | 18 18 20 | 16 18 22 |
| No. of cobs produced.. | 658 | 385 | 205 | 1789 | 262 | 460 | 2486 | 740 | 373 |
| | % | % | % | % | % | % | % | % | % |
| 8-rowed cobs.......... | .3 | ...... | .5 | .1 | ...... | ...... | ...... | ...... | ...... |
| 10 " .......... | 14.4 | 3. | 1. | 1.4 | .8 | .2 | .1 | ...... | ...... |
| 12 " .......... | 48.0 | 22.8 | 13. | 22.6 | 14.5 | 7.8 | 6.1 | 6.1 | 2.7 |
| 14 " .......... | 35.6 | 48.6 | 37.8 | 48.5 | 46.7 | 35 4 | 37.3 | 28.5 | 25.3 |
| 16 " .......... | 3.2 | 18.7 | 34.5 | 22.2 | 23.7 | 33.8 | 33.5 | 41.6 | 41.8 |
| 18 " .......... | .5 | 6.8 | 12.6 | 4.9 | 12.3 | 18.2 | 18.6 | 20.2 | 24.1 |
| 20 " .......... | ...... | .1 | .3 | .3 | 1.2 | 4.4 | 3.9 | 2.8 | 4.8 |
| 22 " .......... | ...... | ...... | .3 | ...... | .8 | .2 | .5 | .8 | 1. |
| 26 " .......... | ...... | ...... | ...... | ...... | ...... | ...... | ...... | ...... | .5 |
| Average .............. | 12.61 | 14.08 | 14.9 | 14.15 | 14.39 | 15.52 | 15.57 | 15.76 | 16.15 |

It will be seen from this table that the number of ears with few rows decreases very rapidly in children produced from seed taken from ears with many rows, and that the greater the number of rows on the ear from which seed is taken, the smaller is the number of ears produced with a small number of rows. It is also plain that as the number of rows on the ear from which seed was taken increases, the number of ears produced with a large number of rows increases, and that we have in each case a very considerable number of ears which equal their parents and a few which excel them, even when the parent seeds are far beyond the maximum of all ordinary corn.

Fritz Müller says that he has never, except in three instances, found an ear with more than 18 rows, and Darwin in his "Variation" puts the maximum at 20 rows, yet we have in the children of seed from a 22 rowed ear no less than 4.8 per cent., or no less than 18 ears out of 373 with 20 rows, and one ear out of 373 with 26 rows. I am quite unable to reconcile this result with Galton's statement "that the ablest children of one gifted pair are not likely to be as gifted as the ablest of all the children of many mediocre pairs." It is undoubtedly true that if Müller had planted in 1869 all the seed from the 2,511 ears which he raised in 1868, instead of planting seed from only three ears, the chance of finding among the descendants ears with 26 or more rows would have been somewhat increased. In this case, however, the parents would not have been mediocre, for nearly all of them were above, and many of them far above, the average for the race, and the chance of finding in ordinary corn an ear with 26 rows is so small that it may be treated as zero.

The results also seem strongly opposed to Galtin's statement that his law tells heavily against the full hereditary transmission of any

rare and valuable gifts, for an examination of the table will show that the number of children which resemble their parents increases in this case with each successive generation. Thus the seed planted in 1867 from an ear with 18 rows produced 12.6 per cent. of 18 rowed children. The 18 rowed ear planted in 1868 from an 18 rowed parent cob produced 18.2 per cent. of 18 rowed children, and the 18 rowed seed planted in 1869 from 18 rowed parents and grandparents produced 18.6 per cent of 18 rowed children. The series is 12.6 per cent., 18.2 per cent., 18.6 per cent.

A percentage of 18 gifted children to the hundred may be discouraging to the "extravagant expectations of gifted parents that their children will inherit all their powers," but it is a most potent factor in the process of race modification by selection.

Müller's table shows, like Galton's observations, that the greatest number of children are not like the parents, but intermediate between them and the "type" or the average for the race. This is exhibited in the following table, in which the number of ears in the parent cob is given in the left-hand column, and the percentage of ears with the same number of rows, produced by the children in the second column, and the percentage of ears produced with the dominant number of rows in the third column.

| | | | |
|---|---|---|---|
| 1867 | 14 | 14 rows 35.6 % | 12 rows 48 % |
| 1867 | 16 | 16 " 18.7 % | 14 " 48.6 % |
| 1868 | 16 | 16 " 22.2 % | 14 " 48.5 % |
| 1867 | 18 | 18 " 12.6 % | 14 " 37.8 % |
| 1868 | 18 | 18 " 18.2 % | 14 " 35 4 % |
| 1869 | 18 | 18 " 18.6 % | 14 " 37.3 % |
| 1869 | 20 | 20 " 2.8 % | 16 " 41.6 % |
| 1869 | 22 | 22 " 1. % | 16 " 41.8 % |

It is thus seen that, like stature, the number of rows tends to revert to the type, but then it will also be seen that, in only three generations, the type itself may be so greatly modified by selection, that the minimum of the third generation may be equal to the mean of the first generation, and the mean of the third generation, 16 rows, is in this case very near the maximum for accidental ears.

W. K. Brooks.

*E'tudes experimentales sur les illusions statiques et dynamiques de direction pour serir a determiner les fonctions des canaux demi-circulaires.* Par Yves Delage. Archives d' Zool. Exper. No. 4, 1886. pp. 535–624, (with index.)

Since the days of Flourens there have appeared few more valuable contributions to the physiology of the sense of equilibrium and of the semi-circular canals than this work of Professor Delage. The author goes far toward reconciling the conflicting opinions of those who, on the one hand, hold that the semi-circular canals are special spatial sense-organs, on whose activity depends every sense of position or direction of movement of the body; and of those who, on the other hand, think there is no good evidence of a normal relation between these organs and the sense of equilibrium. The general question is this: When the eyes are closed, through what sense or senses do we derive ideas of the direction of objects in